while Prof. Young's observations on the sun, now fifteen years old, is the latest information we get in the appendixes on any solar matter, English and foreign work being ignored with a magnificent impartiality. In the same manner Vogel's work on the spectra of stars, the most extensive which has been accomplished by any one single individual up to this time, is also passed over, as is also Birmingham's work on the red stars.

We give these as instances of the treatment adopted. No doubt, had the initial idea of the book been carried out in its entirety by the insertion of the most important parts of these memoirs, the size of the volume would have been greatly increased, and this perhaps may be one reason for the violently selective treatment adopted; but it may be urged on the other hand that the value of the book would have been increased much more than its size, and further, that space might easily have been gained for some of the best modern work by the omission of those papers which, as we have said before, are now purely of antiquarian interest.

There was one feature in the third edition which we also regret very much to see dropped in the present one. This was a bibliography running over twenty pages, in perhaps its most convenient form, namely, a list of authors and a complete reference to their memoirs, arranged under the larger groupings of the subject-matter.

Trigonometry for Beginners, as far as the Solution of Triangles. By the Rev. J. B. Lock, M.A., Senior Fellow of Gonville and Caius College. (London: Macmillan and Co., 1886.)

THIS book covers exactly the same ground as Pinkerton's, which we noticed in NATURE, vol. xxxi. p. 148. The two have many good points in common, and we should be well satisfied to use either of them as a text-book. Mr. Lock's great advantage is preceptorial skill in arrangement and exposition. On this score he deserves much credit indeed. There are very few points on which it is possible to suggest improvement. The retention of the expression "circular measure" in all its former importance, notwithstanding the introduction and constant use of the term "radian," is regrettable but not of much consequence: the mode, however, which he employs for indicating the word "radian," *e.g.* writing π^c for π radians, is most unfortunate, and we should hope altogether unacceptable. It is surprising too to find so skilled a teacher following the multitude in condescending to recognise those unnecessary nuisances, "tabular logarithmic sines," &c. Their existence, Mr. Lock says, is due to a typographic difficulty—a statement we hesitate to give assent to; but, be their history what it may, they serve no purpose nowadays whatever, except to roughen the learner's path. Writers require to give them a foolish name and a special symbol, to alter the formulæ for solution, and to burden the learner with additional cautions,—and all for less than nothing. It seems almost malicious indeed to force on a "beginner" such gratuitous absurdities as "natural sines," "logarithmic sines," and "tabular logarithmic sines," when the entities to be dealt with are simply *sines* and *logarithms of sines.* If Mr. Lock in a succeeding edition could see his way to inaugurate the necessary reform here, many teachers would be grateful to him.

The Apparent Movements of the Planets and the Principal Astronomical Phenomena for the Year 1886. Illustrated with Charts showing the Paths of the Eleven Principal Planets among the Stars. By William Peck, F.R.A.S. (Edinburgh: Archibald and Peck, 1886.)

BEGINNERS in astronomy will find this little compilation useful. Just the kind of information is brought together in it which persons interested, though not learned, in celestial phenomena want to be supplied with Technical language, too, is as much as possible avoided, while sufficient exactness for the purpose in view is usually preserved. Not, however, invariably; the statements regarding the two solar eclipses visible in 1886 are so loose as to be misleading. Eleven miniature maps, showing the paths through the constellations during the present year of seven primary and four minor planets, are neatly executed, and ought to prove acceptable to casual observers. Exception must be taken to the introductory assertion that Copernicus swept away all the "complicated machinery of the heavens." His reform of the Ptolemaic system was by no means so complete as Mr. Peck's expression implies. The retention by the Frauenburg astronomer of the old hypothesis of equable circular motion necessitated, in fact, the employment still of no less than thirty-four circles, by which to make plain, as he said, "the entire structure of the heavens"—that is, the revolutions of the moon and of the six known planets.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

An Earthquake Invention

IN NATURE of July 2 last, p. 213, I was accused by Prof. Piazzi-Smyth and Mr. D. A. Stevenson of having attempted to appropriate an invention of Mr. David Stevenson. The invention referred to was the joint introduced by Mr. Stevenson beneath the lamp-tables in certain lighthouses in this country.

In my reply (NATURE, vol. xxxii. p. 573) I pointed out the fact that the aseismatic joint had been independently invented by several investigators of earthquake phenomena, and so far as I was aware Mr. Mallet had appeared to have the prior claim to this invention. My reason for attributing the invention to Mr. Mallet is that when speaking of Japanese lighthouses he says: "I was consulted by Mr. Stevenson as to the general principles to be observed, and these edifices have been constructed so that they are presumedly proof against the most violent shocks likely to visit Japan; not perhaps upon the best possible plan, but upon such as is truly based upon the principles I have developed" (Palmieri's "Vesuvius," p. 43). As the aseismatic joints were portions of the lighthouses especially designed to render them proof against earthquakes, I naturally assumed that Mr. Mallet might be the first inventor of the ball-and-plate joint.

The only occasion on which I have posed as *the* author of the aseismatic joint in question, was when Messrs. Stevenson and Smyth promoted me to that *quasi*-enviable position.

Had these gentlemen recognised the fact that they were only reading a *brief note* about ball-and-plate joints, intercalated in a collection of notes on other subjects, and had they been well acquainted with the recent literature relating to aseismatic tables, they would certainly have refrained from the objectionable accusations made on July 2.

On *more than one* occasion I have referred to Mr. Stevenson's work in Japan. As an example of such a reference, Messrs. Stevenson and Smyth may turn to the *Times* of May 26—*a date which it will be observed is prior to the date of their unwarrantable attack.* In that paper there is a long letter on "Buildings and Earthquakes" signed with my name. When speaking of my house on shot, I there say, "*This experiment was very similar to one carried out by Mr. David Stevenson with regard to the lamp-tables in several of the lighthouses on the coast of Japan.* For several reasons, among which were the movements produced by wind, I abandoned the balls, and now have my house resting at each of its piers upon a handful of cast-iron shot. These shot, which are about the size of buckshot, have so increased the frictional resistance to rolling, that the house is practically astatic, and the motion in the house is in most earthquakes only about one-tenth of what it is outside."

I make especial reference to the *Times*, first because it is a

paper which many Englishmen have often the opportunity of seeing; and second, because the article in question occasioned considerable discussion.

With this reference to the relationship of my experiments to those of Mr. David Stevenson, and I will furnish others if required, I think it would only be just for Prof. Piazzi-Smyth and Mr. D. A. Stevenson to withdraw their accusation of July 2.

I must now refer to a second point which Mr. Stevenson has raised. In your issue of August 6 Mr. Stevenson appeals to the results of my experiments as showing the value of his aseismatic tables. All my experiments with aseismatic tables, whether used for seismographs or buildings, when approximating in form to Mr. Stevenson's joint were, for a variety of reasons, unsatisfactory. Under compulsion I have stated that Mr. Stevenson's joint itself, as applied to the lighthouses of Japan, has been unsatisfactory. I did so for the following reasons:—

(1) Shortly after erection the free motion of the tables occasioned so much inconvenience, that the European engineers then on the Japanese service had them clamped. For this reason Mr. Stevenson's arrangement was not adopted in lighthouses which were subsequently erected (see Brunton on "The Japan Lights," Institute of Civil Engineers, No. 1451, p. 9).

(2) I learn from the Lighthouse Department that in 1882, wishing to give Mr. Stevenson's tables another trial, several of them were put in working order. The result has been that on March 11, 1882, at Tsurgisaki, a number of lamp-glasses on the burners on the aseismatic tables were overthrown.

Some time afterwards a second shock produced a similar effect. At neighbouring lighthouses, of which there are several (two being within 8 miles), not provided with aseismatic tables, no damage was sustained.

The shock of March 11, 1882, which was felt for at least 300 miles along the coast, was severe, and its effects at Yokohama and Tokio, which are no great distance from Tsurgisaki, were carefully recorded. I am not aware that any small articles like lamp-glasses, bottles, vases, &c., were overthrown inside ordinary houses (see *Trans. Seis. Soc.*, vol. vii. part ii. pp. 41–44). The fact that no ill effects occurred at other lighthouses provided with Mr. Stevenson's tables, like those on the Inland Sea and near Kiushiu, must not be regarded as an argument in favour of the tables, inasmuch as the earthquake referred to was not felt in those districts.

I may here remark that several of the aseismatic tables are at places where earthquakes are almost as rare as they are in Britain. Mr. Stevenson tells the readers of NATURE (June 26) that his lighthouses suffered when the aseismatic tables were *not* in working order. I have shown that they suffered when they were in good working order.

Speaking generally about Mr. Stevenson's descriptions of his aseismatic arrangement, he invariably refers to it as a great success. Where it was applied, earthquakes have produced no effect, but where it was suppressed, evil effects have resulted. After inquiries at the Lighthouse Department, which is a branch of my own department, I find that the facts adduced by Mr. Stevenson are exactly the reverse of the facts which have been placed at my disposal; and from what I learn, and from my own experiments, I conclude that thus far Mr. Stevenson's tables have been a failure.

As a further support to my conclusions I will quote the following translation of a report from the chief lightkeeper at the Tsurgisaki Lighthouse:—

"Sir,—On October 15, 1884, at 4.16 a.m., very severe shocks of earthquake were felt. The aseismatic table was in working order, but the shocks were so violent that fifteen lamp-glasses out of twenty-one in use were upset and broken. The lamps thus stripped of glasses began to smoke. The milled heads of the wick-holders being shaken off, and besides the revolving machine being in motion, we had some difficulty in replacing the glasses promptly; however, we managed to put them all in proper order again by 4.21 a.m.—I am, Sir, your obedient servant, &c., &c."

The only form of aseismatic joint that I have found at all practical is one where something more like a layer of cast-iron sand rather than a bed of cannon-balls is used to break the continuity between a structure and its foundations. I arrived at this after spending much time and, I may add, a considerable sum of money, and although the method involves the same *principle* as Mr. Stevenson's tables, I hardly think he is justified in claiming my successes to back up what I cannot but feel have been his failures.

If the ways and means permit, I hope to make experiments upon a small building resting on a bed of sand or fine gravel. Should results of any value be obtained, surely Mr. Stevenson will not expect me to do more than I have done already—namely, to state the relationship which may exist between these experiments and those which he carried out at the expense of the Japanese Government.

If everything connected with earthquake investigation which involves the same *principles* as are involved in Mr. Stevenson's lighthouse tables are to be regarded as his creations, he cannot avoid claiming the rolling sphere seismograph, the rolling cylinder seismograph, the horizontal pendulum seismograph, the conical pendulum seismograph, and in short, a very large proportion of the work accomplished by the Seismological Society of Japan. To this I cannot assent. All that Mr. Stevenson can be accredited with is a particular *method* of applying a principle, and this method has to my mind been a failure.

The question of the *principle* involved in Mr. Stevenson's device is one that has been *repeatedly* discussed in Japan. As an example of these discussions I will refer to the *Transactions* of the Seismological Society of Japan, vol. iii. p. 9, where Prof. J. A. Ewing is speaking, amongst other things, about a rolling sphere seismograph, the invention of Mr. Thomas Gray. Prof. Ewing says that Mr. Gray's contrivance was an application of "the method of supporting a mass by a movable piece in such a manner that the connection with the earth was at the centre of percussion of the movable piece, the mass being at the corresponding centre of instantaneous rotation, while at the same time the supporting piece was arranged so that its movements did not introduce any disturbing force due to the action of gravity upon the mass. This kinetic property, common to all these instruments, he believed he might fairly claim to have introduced into seismometry."

If Messrs. Stevenson and Smyth see fit to comment upon these notes, I trust that they will distinctly state whether they yet consider that I have attempted "to get round the letter" of Mr. David Stevenson's invention, and whether they were justified in publishing the objectionable personalities about "a B.A. man" on July 2.

This is the main point at issue, and if they choose to neglect it, the discussion may be considered as ended.

In conclusion I may remark that it was not I who commenced this controversy. JOHN MILNE

Tokio, January 6

On the Velocity of Light as Determined by Foucault's Revolving Mirror

A FEW years ago Lord Rayleigh raised an interesting question as to the quantity actually determined by our experiments and observations on the velocity of light. There can be no difficulty as long as the medium transmits different wave-lengths with the same velocity, but whenever the medium possesses the property of dispersion the velocity with which any one crest of a wave travels (V) is different from the velocity with which a group of waves is propagated (U); hence the question arises in each particular case, whether it is V or U or something depending on both quantities that we measure.

In his first article on the subject (NATURE, vol. xxiv. p. 382) Lord Rayleigh states that, in experiments with Foucault's revolving mirror, the group-velocity, U, is determined; but subsequently (vol. xxv. p. 52) he corrected this statement and gave V^2/U as the quantity measured. A paragraph was added, however, in which the remark is made that, if a convex lens is interposed so that an image of the slit is formed on the fixed mirror, the rotation of wave-front, caused by the different velocities of different wave-lengths, and acquired on the outward journey, is neutralised during the return, so that in this case we should measure V.

Gouy (*Comptes rendus*, ci. p. 502, 1885) dissents from Lord Rayleigh's second view, and gives U as the quantity determined; without, however, giving sufficient reason in support of his opinion.

Finally, Michelson has performed the experiment with bisulphide of carbon, and obtained a result in close agreement with U. In a discussion of Michelson's measurements in the *American Journal of Science*, by J. W. G., his result is said to give "no countenance" to the theory which would make the velocity observed V^2/U.

It is the object of this communication to support Lord Ray-